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J. Lederberg.
and E. Nester

20th August, 1962.

Dear Josh

So far as relevant
not unavailable

I have at long last (and I am much abashed at the excessive delay) completed a draft about the phenotypic expression and micromanipulative isolations of cells of transformed phenotype which I did at Stanford. I have rather forgotten what the last arrangement proposed about publication was; but when I got down to serious drafting it seemed clear to me that it would be better to write one paper about the above topics, over my name only, just mentioning the penicillin experiments and the inference as to the "latency" of competent/transformed cells; and to reserve for a separate paper, by Nester and Stocker, the kinetic experiments in liquid (Nester and Comstock, and later ones), my kinetic experiments by re-spreading and replication, our data on the penicillin resistance of transformants, Gene's results ~~results~~ on experiments in which penicillin was added first, then DNA (if they should go in), also my data on the failure of the per cent of accidental double transformat~~ion~~ts to decrease during post-DNA incubation in complete medium, etc.; all leading to the conclusion that competent, or perhaps transformed, cells in the B. subtilis system are anabolically inactive for several hours; also to show the relevance of this to the proportion of doubles being higher than the product of the proportion of singles in this, but not I think in other, transformation systems; and, maybe, a brief note on the results of re-extraction of DNA from DNA-treated competent cells, to show that synapsis, and perhaps crossing-over, occur during the period of latency.

Anyway I have completed a draft on the expression and micromanipulation results, and I shall have sent off a copy to you second class air mail ~~at the same time as this letter~~. I realize that the text needs a good bit of cutting, tidying up and correction of a few disagreements with the tables and figures which I think are correct. I would be very glad if you would look this over, and

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let me have corrections, comments and suggestions. Also any suggestions as to the best journal to submit the paper to, when it has finally been fixed up and approved. As I did the work in the United States I think that an American journal would be appropriate, but I do not know which one would be suitable. The Journal of Bacteriology perhaps if I could shorten the paper sufficiently for them. (The J. gen. Microbiol. has an exceptionally big back-log at the moment, I hear). I apologise for not sending the typescript in more finished form; but I am writing against time before I go off on August 22 to Luria's meeting in Vermont; and a few days at Cold Spring Harbor before, for the phage meeting and to compare notes on LT2 mapping etc. with Norton, Zinder, Demerec, Hartman, etc.) I also hope to spend 2 days at Boston afterwards, and to be back in London on September 1.

I have half finished drafting an outline or skeleton for the proposed joint paper on the "latency", and I shall try and get this completed and sent off also before I go. But arranging the papers in this way means that the latency paper will have much more of Gene's results in it than of mine, so I just propose this outline for your consideration, Gene.

I was invited by Guinnesses to go to Dublin last month to the Genetical Society Meeting; and I took the opportunity to give a paper on my Stanford results, including both the penicillin and the isolation data. I shall send with the typescript a copy of the abstract which will ultimately appear in the Genetical Society Proceedings in "Heredity" (and, I hear, also in "Annals of Human Genetics"). I hope you approve of what I have said in the abstract and will not mind that I did not consult you first, Gene.

David Dubnau, who is working with Pollock, (his wife is doing her Ph.D. here) is trying to get transformation in respect of penicillinase production etc. He says that strains 168 and W 23 produce not detectable penicillinase (though he has not yet used the most sensitive technique available) and that crude DNA from his other strains, of independent origin, does not effect ind+ transformations in strain 168. So he is going to try a range

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of other pen-res+ on DNA donors strains. I have no news on the B. subtilis front, except that in a few experiments my visitor Lhoas, who has now returned to Belgium, only managed to obtain rather mediocre competence. In a few experiments with Thorne's transducing phage he obtained reasonable transduction of ind+, with the rate of co-transduction of ind and his, being considerably higher than the rate of co-transformation; but he did not succeed in getting any transduction of motility to SB108. Have you any experience of any of the other transducing phages that I hear other people have isolated ?

Yours sincerely,

Bruce.

Bruce Stocker.

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Dr. E. Nester,
c/o Lederberg

and

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Is Gar coming this way? Esther said he might: & we have a letter addressed him here, but no other news. I expect you heard Rogeen is on way back to States, as we could not fix her up here; nor with Francis Crick, & she wasn't too keen to shop around for jobs.